of 6 (= 7 forms) to each, *P. major* and *P. palustris* each having 13, *P. ater* 14, etc.

The Alaskan *Budytes flavus alascensis* Ridgw. stands as *Motacilla flava alascensis*; the North American *Anthus pensylvanicus* as *A. spinoletta pensylvanica*.

Eremophila is used in place of *Otocoris*, since in the opinion of Mr. Hartert the generic names *Eremophilus* and *Eremophila* are both tenable. It may here be also noted that from his point of view a specific or subspecific name need not agree in gender with the generic name, it being his preference to preserve the original ending of a specific name when transferred to a genus which has a different gender ending, as in the case above of *Anthus pensylvanicus*, which was originally described as a species of *Alauda*. It may be further noted that the palæarctic species of *Otocoris* here recognized number 15, all subspecies of *alpestris*, as against 14 recently admitted by Oberholser, who, however, gave full specific rank to 5 of them. But in only nine cases are the same names adopted.

Despite certain excentricities of treatment, Dr. Hartert's 'Die Vögel der paläarktischen Fauna' will long prove a most useful and convenient hand-book, for which ornithologists may well feel deeply grateful.— J. A. A.

Clark on the Amount of Difference that should characterize Species and Subspecies.— We regret that the character of Mr. Clark's paper[1] is such that if it is to be noticed here at all it must be considered at some length. Were it not that it doubtless reflects the attitude of the 'lay' class, and thus appeals to the sympathies of the untrained who have neither the opportunity nor, perhaps, the desire to become experts, and is thus a misleading presentation of the case, it might well be passed over without mention.- The author, Dr. Hubert Lyman Clark, is not unknown to readers of 'The Auk' and to ornithologists in general through his various excellent papers on the pterylography of various groups of birds, but so far as technical descriptive ornithology goes his experience has evidently been extremely limited. That such is the case, the rules he prescribes give evidence.

In the present paper he has formulated "fundamental rules," which, it seems to him, ought to govern work in systematic zoölogy. They are each explained and defended at some length against criticisms made by the present reviewer upon a previous paper of his on the same subject. The history of the case cannot be given better than in his own words. As the following quotations contain transcripts of the previous objectionable criticisms they will in part cover what it seems desirable to say in the present connection. He says:

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[1] The Limits of Difference in Specific and Subspecific Distinctions. By Hubert Lyman Clark. Fifth Annual Report of the Michigan Academy of Science for the year 1903, pp. 216–218.

"It was my misfortune last summer to feel called upon to criticise some recent ornithological work in which the process of recognizing subspecies had been carried to the extreme, and my opinions were published in SCIENCE, August 8, 1902, under the heading 'So-called Species and Subspecies.' In the same journal, September 5, Dr. J. A. Allen, the well known zoologist, criticised my opinions as those of a layman, and emphatically denied two of my main contentions. As nothing is gained by newspaper controversy, I made no reply, but the questions involved are extremely important and after six months further consideration of them, I have decided to set forth what seem to me some of the fundamental rules, which ought to govern work in systematic zoology. First, however, as Dr. Allen has challenged my right to opinions on the subject, it is only fair to say that, although I have never described a new or supposedly new bird, I have had occasion to examine carefully several thousand specimens of echinoderms, and have been under the necessity of naming a number of new species in that group, so that I am not an entire stranger to the numerous perplexities of the systematist, to which Dr. Allen refers. Now I freely admit that from the systematist's point of view, birds are more perplexing than echinoderms, and that Dr. Allen, both because of his naturally judicious temperament and by his many years of experience amid exceptional opportunities, is far better qualified to discuss this subject than am I. Yet I do feel, that whether the animal be a bird, a fish, a worm or an infusorian, the essential principles of systematic zoology ought to be the same in all cases, and that any zoologist who has wrestled honestly with the knotty problem of specific distinctions is entitled to opinions on the subject. I therefore venture to state some of these essential principles as they appear to me.

"*1. Characters which are not sufficiently conspicuous, so that they can be stated in language or figures of some sort, ought not to be made the basis of a new name.*

"This principle appears so axiomatic that an apology would be made for stating it here, if it had not been seriously questioned by Dr. Allen. He says: 'In ornithology, and especially in mammalogy, perfectly "good species" are often so similar in size and color that even the expert cannot satisfactorily identify them from descriptions, and hence, almost from time immemorial, direct comparison with authentic material has been necessary in order to settle such difficult cases. As all experts in this line of study well know, forms that may be indistinguishable by descriptions are, when brought together, and especially when series are compared, so noticeably different that there is no trouble in distinguishing them at a glance.' Now I confess that after giving these words careful thought I am unable to believe that the validity of my contention is affected. I am utterly unable to conceive of two objects, which I could 'distinguish at a glance,' the differences between which would be so intangible that I could not state them 'in language or figures of some sort.' As to the comparison of specimens with types or other authentic

specimens, 'from time immemorial,' surely it is well known that the necessity for this is due to imperfect, inaccurate and erroneous descriptions, and not to the fact that 'perfectly "good" species' cannot be distinguished without comparison. If a character, whether in color, size, form, texture, odor, notes, habit or anything else, cannot be detected by sight, touch, smell, taste or hearing to such a degree as to admit its translation into intelligible language or figures, it surely is not fit to be made the basis of a new name. Of course I do not contend that the 'language or figures' must be intelligible to the 'layman,' for that unfortunately is not at present feasible and probably never will be."

In regard to "imperfect, inaccurate and erroneous descriptions," it may be said that such we have always with us, and always will have; they almost form the bulk of past descriptive zoölogy, and will hold also a prominent place in the future; they are partly, perhaps largely, due to carelessness and slovenly methods, but are in part inherent and unavoidable, until a standard terminology for shades of difference in colors shall have been invented and generally adopted. Language at present is inadequate to convey to the mind definite and exact shades of color, even when strikingly different to the eye, because scarcely any two persons would describe the same shades between, say buff and chestnut, running through the endless tones of yellowish and reddish browns, in just the same terms. Whatever the cause of this vagueness of description, it exists, and will exist for a long time to come, producing a condition militant against Mr. Clark's 'Principle 1.' Words, as we now have to use them, cannot adequately convey to the mind differences in color and texture that are palpable enough when seen.

"*2. Differences in dimensions, of less than five per cent., ought not to be made the basis of a new name.*

"This principle is certainly not radical, yet it would shut out a large number of recently described subspecies of birds, and perhaps other animals also. The reason for this rule is that individual variation in a species is so much larger than was formerly supposed, no constant difference can be maintained between two forms which differ from each other by less than five per cent. in size. I believe ten per cent. would be a safe rule, but if five per cent. could be agreed on many ridiculous new names would never see the light of day. In Dr. Allen's famous paper 'On the Mammals and Winter Birds of East Florida' (Cambridge, 1871), he says: 'The facts of the case show that a variation of from fifteen to twenty per cent. in general size, and an equal degree of variation in the relative size of different parts, may be ordinarily expected among specimens of the same species and sex taken at the same locality, while in some cases the variation is even greater than this.' Such being the case five per cent. is not a high standard to suggest."

While Mr. Clark's quotation from my 'Mammals and Winter Birds of East Florida' respecting individual variation is all true, there is another side to the question, and that is that the average difference in general size

or the size of particular parts, as the bill, wing, tail, tarsus, etc., in closely related species is often much less than the range of purely individual variation in any one of the several species that may be involved ; and where color at the same time may fail, as happens in some groups, even the expert is sorely puzzled to discriminate between museum specimens of species that in life are at once recognizable as distinct by their notes, habits, pose, and almost every act and attitude, as in the case of some of the species of the genus *Empidonax*. Thus an expert confessed to me that on one occasion when he came to label up his season's collecting he found that in order to tell 'tother from which' in the case of two perfectly distinct species of *Empidonax* he had to resort to dates of collecting and his notes on the living birds entered in his notebook to decide which was which! The present writer once had also a similar experience. Yet it is not quite impossible nearly always to recognize these closely related forms — good species, not subspecies — without recourse to notes on the living bird. Much more might be said anent "Principle 2"; but inasmuch as many species that no one could confound in life would be ruled out by the "five per cent." rule, it is hardly necessary to say more.

"*3. Characters which cannot be recognized without knowledge of the geographical origin of the specimen ought not to be made the basis of a new name.*

"This is a very essential principle if we agree that an important end of systematic zoology is correct knowledge of the geographical distribution of animals. It seems to me axiomatic that characters which cannot be recognized regardless of the locality where the specimens are collected are worthless, yet Dr. Allen holds to the contrary, and regards my support of this principle as evidence of my writing without possessing the necessary familiarity with the facts. The horned lark from Mexico named *Otocoris alpestris chrysolæma* by Oberholser differs from the same author's subspecies *actia* so slightly that he himself admits they are indistinguishable, unless the locality where collected is known. I am unable to see what possible gain there is in giving a name to such a form ; while christening it may easily lead to serious errors in determining the geographical distribution of the real subspecies of horned larks. And in all other groups of animals, the confusion of special geographical position with essential morphological character leads, and always will lead, to most erroneous conclusions concerning the distribution and history of species. A well known American mammalogist is said to hold the view that any mammal resident on an island must necessarily be a different subspecies from the form on the neighboring mainland, because of its isolation. If such views are current among systematists, (which I greatly doubt), it is not strange that morphologists, physiologists and embryologists have long held systematic zoology in contempt, and even now regard with suspicion our claims to a place among the real devotees of science."

Respecting 'Principle 3' little need be said, either in its favor or

against it. It may stand as at least a suggestion worthy of serious consideration. We confess being to a large degree in sympathy with it and with much of the author's comment thereon. It is to be noted, however, that in the case of closely related subspecies, the normal range of individual variation might make it impossible to properly refer occasional specimens without a knowledge of the locality of their origin ; and probably more than once has the mistake been made of recording a western race from an eastern locality on the basis of an aberrant eastern bird.

In regard to slightly differentiated insular forms, it was at one time quite generally held that as there could not be actual intergradation between such forms and the mainland stock (in the case particularly of the smaller land mammals), owing to their physical isolation, it was better to recognize such forms as species than as subspecies, since the latter are either known or assumed to intergrade through the continuous range of the geographical forms of a widely dispersed species. Of late, however, this method is being largely abandoned, slightly differentiated insular forms being now very generally treated as subspecies.

Now that Mr. Clark has voiced the 'contempt' long held by morphologists, physiologists and embryologists respecting the work of systematic zoologists, the retort may be made that the contempt is, to a certain extent, mutual, and perhaps to some degree not without cause on both sides. But only the most narrow-minded of either class can fail to recognize good work outside of their own circumscribed specialities. A certain class of the 'section-cutters' take little account of the broader relations of animals to their surroundings, and in their histological and statistical investigations have been known time and again to work on a lot of heterogeneous material under the impression that it was all comparable and homogeneous,— as conspecific while in some instances it was not even congeneric, to say nothing of generalizations under statistical methods based on incongruous and non-comparable material.

"*4. Characters which will not distinguish corresponding ages or sexes of two forms ought not to be made the basis of a new name.*

"This seems so self-evident, I hesitate to state it, but as it may prove the one on which we can all agree I mention it, although it is no more obvious to me than principles one and three. Of course this does not mean that the characters must be present in both sexes at all ages. On the contrary, the characters may be present only in one sex or at a particular time of life, but they must distinguish from the corresponding sex or age."

The author's comment under 'Principle 4' to some extent explains its intent, without which it would be quite absurd. Thus: "Characters which will not distinguish corresponding ages or sexes of two forms," etc.; but he evidently does not mean this to apply to species in which, while the adult males are too distinct to be confounded by the merest tyro, the females and young cannot be positively discriminated by the expert; or in other cases where, while the females are distinguish-

able, the adult males are indistinguishable. As it stands, even with the explanation, it has little relevancy or raison d'être.

"*5. Characters which are notoriously variable in a given group ought not, within that group, to be made the basis of a new name.*

"As an example of what is meant by this principle, the common starfishes (*Asterias forbesi* and *vulgaris*) of the New England coast may be cited. Their color is so remarkably variable that it would be folly to form subspecies based upon the color alone.

"*6. Characters which may be fairly interpreted either as individual peculiarities or as dichromatic diversity, ought not to be made the basis of a new name.*

"If this principle were honestly followed many new species and subspecies would be cancelled, and it would lead to much greater caution in basing new names upon single individuals.

"The above six principles are suggested, not with any idea that they will meet with universal approval, but in the hope that they may precipitate a discussion which will lead to definite results. At some not far-distant day let us trust, the charge of basing new names upon 'distinctions without a difference' will be one that cannot be brought legitimately against American zoologists."

Respecting "5" and "6," little need be said; they are certainly harmless, if not very helpful, for no experienced 'systematist' is likely to violate either.

The concluding paragraph of Mr. Clark's paper is given as clearly showing his good intentions. This republication of his paper in full, and the running comment thereon, may be taken as an attempt to comply with his desire that it "may precipitate discussion which will lead to definite results."—J. A. A.

Mascha's 'The Structure of Wing-Feathers.—This is an account[1] of an investigation by Dr. E. Mascha, under the direction of Professor R. von Lendenfeld of the Imperial German University in Prague, with the object of giving "a detailed account of the morphology of the wing-feathers of birds as used in flight," made with the hope of supplying "needed and valuable information for those interested in the great problem of aërial navigation." It is based on the examination of the quills of about 25 species, belonging to about a dozen orders, and comprising birds of most types of flight. Their histological structure is described in detail, and illustrated by figures grouped to form 16 plates. The text has apparently suffered in translation from the original German manuscript (to be published in the 'Zeitschrift für wissenschaftliche Zoologie,' here and there occurring terms and sentences by no means clearly expressed. At the end of the paper a 'summary of results' is given, in sixteen short para-

[1] The Structure of Wing-Feathers. By Dr. E. Mascha. Smithsonian Misc. Coll., Quarterly Issue, Vol. III, pp. 1–30, pll. i–xvi. May 6, 1905.